PERIODICITIES, SOLAR AND METEOROLOGICAL

551.590.2(048)

By C. CHREE

[Reprinted from Science Abstracts, 1924, no. 2688. See Jour. Roy. Meteorolog. Soc., 50, pp. 87-97, April, 1924]

An address, delivered before the Royal Meteorological Society, in the first part of which sun-spot frequencies for the years 1856 to 1921 from Wolfer's lists are compared by various methods with mean yearly values of data relating to Kew Observatory, comprising rainfall, mean temperature, daily range of temperature, sunshine, cloud, mean potential gradient, absolute daily range of magnetic declination, and diurnal inequality range of declination and horizontal force for such periods as they are available. The points which the writer wishes to bring home to meteorologists are the two facts: (1) That an important sun-spot relation does exist in at least one terrestrial element—terrestrial magnetism; (2) that, at least in these latitudes, the evidence for a connection between sun-spot frequency and meteorological phenomena is of quite a different order from the evidence for a connection between sun-spot frequency and terrestrial magnetism.

The considerations put forward amply illustrate the danger of drawing conclusions in such matters as the present from data extending over an insufficient period of time. In the latter part of the paper the author is able, however, to suggest for trial a method by which relationships between sun-spot frequencies and terrestrial elements might be investigated, even though data for the latter may not be available for a number of 11-year periods. Reasons are given for supposing that the difference between magnetically quiet and disturbed days is essentially of the same nature as the difference between quiet days at sun-spot minimum and quiet days at sun-spot maximum. Granting this, any meteorological or electrical element which exhibits the 11-year period will, it is reasonable to suppose, behave differently on days that are magnetically quiet and on days that are magnetically disturbed. The suggestion is, then, that the meteorological data relating to the two classes of days, as well as the electrical data, should be compared at a number of places, it being noted in this connection that the international lists issued from De Bilt will in future specify five disturbed days a month as well as five quiet days. It is shown to be desirable to commence with places in high magnetic latitudes as being most likely to yield best results.—*M. A. G.*

TORNADO CLOUDS

By CLARENCE J. ROOT

[Weather Bureau, Springfield, Ill., December 1, 1924]

An article by Mr. Varney in the August, 1924, number of the MONTHLY WEATHER REVIEW discusses the tapering shape of the damaged area in connection with the tornado at Lorain, Ohio, June 28, 1924. Figure 2 shows the relation of decreasing area at the earth's surface to the rise of the funnel cloud.

This brings to mind the so-called Mattoon tornado of May 26, 1917. The path of this storm was perhaps the longest of record, extending across the entire State of Illinois and three-fourths of Indiana, a distance of 293 miles. Across the State from the Mississippi River almost to Mattoon all eyewitnesses agreed that this storm had the typical funnel-shaped tornado cloud with swinging tail, and east of Charleston the same type of cloud was reported, but the writer who visited Mattoon and Charleston, failed to find anyone in those cities who saw a funnel-shaped cloud. Eyewitnesses who had an unobstructed view agreed that the approaching storm appeared as a low, boiling mass of clouds, one part a little to the north and the other a little to the south. The parts seemed to roll toward one another, coming together and downward like the meshing of a pair of cog wheels. In the official report it was suggested that the cloud was so low that there was no room for the usual pendant portion. The path of serious damage was generally about one-fourth mile in width.

There was ample evidence of tornadic action in Charleston and Mattoon. A barograph trace showed a tornado dip, buildings "exploded," the walls falling outward, and the directions in which the trees lay were typical of the true tornado. The direction of movement was a little north of east. The southern limit of the zone in which the trees fell to the west coincided exactly with the northern limit of the zone of complete destruction. This indicates that the greatest wind force occurred on the south of the actual center of the whirl.

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HUMIDITY RECORDERS

By E. B. WHEELER [1]

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The material effect of atmospheric conditions upon the operation of intricate electrical and mechanical apparatus, such as is found in telephone systems, is discussed, and the desirability of obtaining accurate information as to the character of the atmospheric conditions obtaining in typical localities is emphasized. The specific advantages and limitations of various well-known types of hygrometers are reviewed and the development of a new recording hygrometer is described in which the Leeds and Northrup automatic recorder is employed in conjunction with wet and dry bulb resistance thermometers in an auxiliary wind-tunnel equipment and a specially designed double Wheatstone bridge circuit. One of the Wheatstone bridges contains the dry bulb thermometer, the other containing both the dry and wet bulb thermometers. The satisfactory performance of a number of such instruments is illustrated in typical graphs. A new direct-reading humidity recorder is also described working on a similar general principle, and depending for its operation upon the approximate linearity and common intersection of the ordinary humidity curves connecting wet and dry bulb temperatures for a given relative humidity.—*A. B. C. L.*

EDITOR'S NOTE.—An examination, by the Instrument Division of the Weather Bureau, of the original paper, indicates that the recorder described is probably more precise than any other which has come to the attention of the division. On the other hand, it is believed to be too expensive and complicated for general meteorological work. Readers of the REVIEW will, however, be interested to know of the existence of this instrument of high precision.

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THE CAUSE OF CYCLONES

By A. H. R. GOLDIE, Edinburgh

[Reprint from Nature, 114: November 29, 1924, pp. 786-787]

In the winter of 1922–23 there appeared in Nature some correspondence on "The cause of anticyclones," and on that occasion I put forward certain views as to the mechanism by which the more rapid increases

[1] Bell System Techn. Jour., 3, pp. 238-258, April, 1924.

of barometric pressure are brought about in temperate latitudes (Nature of March 31, 1923, pp. 429–430). The present communication may be regarded as a sequel in that its object is to describe a mechanism by means of which the more rapid reductions of pressure can conceivably be produced. The idea arises naturally from consideration of a series of papers by Helmholtz appearing in the Sitzungsberichte of the Royal Prussian Academy of Sciences in 1888 and 1889, dealing with the equilibrium of rotating rings of air at different temperatures, and with the theory of winds and waves where strata of different density lie contiguous with one another.

The conditions for equilibrium in cases where a surface of discontinuity (of temperature and wind) exists in the atmosphere have since that time been dealt with by V. Bjerknes and others in somewhat more general fashion and need not be set out here.

Designating the warmer (southerly) mass by (1) and the colder (northerly) by (2), Helmholtz calculates in terms of their velocities, temperatures, and the latitude the theoretical slope of the bounding surface for a state of stable equilibrium. But, as he points out, first small waves and then mixing of the two media must soon occur over this boundary; he therefore calculates the slope for stability of the bounding surface between mass (2) and the mixture and between the mixture and mass (1). He shows that these slopes are respectively more acute and less acute (relatively to the horizon) than the original slope between mass (2) and mass (1). Hence, he says, results the important consequence that (in tending toward the new requirements for equilibrium) "all newly formed mixtures of strata that were in equilibrium with each other must rise upward between the two layers originally present, a process that of course goes on more energetically when precipitations are formed in the ascending masses. While the mixed strata are ascending, those parts of the strata on the north and south that have hitherto rested quietly approach each other until they even come in contact, by which motion the difference of their velocities must necessarily increase since the strata lying on the equatorial side acquire greater moment of rotation (about the earth's axis) with smaller radius, while those on the polar side acquire feebler rotation with a larger radius."

Now the importance of this reasoning, applied in the light of modern meteorological knowledge, appears to me enormous. Putting aside for the moment all theories as to the origin of cyclones, it appears to be pretty well established by the facts of observation that the normal structure of an active and recently formed depression at least approximates to that described by Bjerknes.

Further, observation tends to confirm (1) that the principal reduction of pressure and the principal area of rainfall lie at any moment within the "old" cold air in front of the Bjerknes steering line, (2) that where the warm air extends right down to the ground no appreciable pressure-change is taking place, unless quite close to the steering line, (3) that, where the "new" cold air is undercutting the warm air, there is rapidly rising pressure.

I have shown elsewhere that the rise of pressure in the new cold air and the comparative constancy of pressure in the warm air and also the temperatures of these masses are consistent (dynamically) with their (supposed) northerly and southerly origins and with the processes to which they have been subjected in arriving on the field of operations; and further that the changes in the energy (potential, kinetic and internal) of the "new" cold current are sufficient to account for the work that is being done by it in displacing the warm air and filling up the rear of the depression. But I have never previously been able to see why in front of the depression the warm air should mount upon the old cold air, or why, if it did so, a fall of pressure should at the same time occur in the region below; though I have indeed been able to find evidence that a selected mass within the warm air was continually approaching a selected mass within the old cold air. Now, if the Helmholtz reasoning is sound, we can make an important step forward. The mysterious "eviction of air," as Sir Napier Shaw has called it, is accomplished by the continual procession of the products of mixing of the warm and cold up a kind of giant escalator—with a moving roof as well as steps—hitherto described usually as the steering surface; and the process would go on so long as any warm sector remained.

At the moment, I can see no fact of observation that is inconsistent with such a theory, and it further appears to lead to feasible energy equations for the front half of a cyclone.

In the problem dealt with by Helmholtz the "eviction" and the subsequent "dumping" of the air do not seriously enter, and beyond the remarks quoted above, he devotes no consideration to either. As to the latter, steering surfaces appear to extend up to the level of the highest clouds and even into the stratosphere, and their inclination to the horizon appears to be of the order of 1:100. Hence the dumping need not take place within 1,000 kilometers of the steering line and may be spread over an enormous area beyond that. The tracks of the most frequent North Atlantic depressions lie roughly parallel to the line of the (eastern) American and Greenland coasts, and in a belt about 1,000 to 2,000 kilometers distant from these coasts. The tracks of the North Pacific depressions are somewhat similarly situated with regard to Siberia. Of all places in the Northern Hemisphere the North America-Greenland area and Siberia would most welcome the dumping of surplus air and most of all at those seasons when depressions are most active. For these areas must be receiving air—probably at a high level—to compensate for the constant vertical contraction of the film of atmosphere there by cooling and for the recurrent discharges of cold air into the rear of passing depressions. In this way depressions might perhaps be regarded as the heat-receiving ends of thermosyphonic arrangements, the corresponding radiating ends being areas like North America, Greenland, Siberia, or the Antarctic continent; both ends being necessary for the maintenance of the circulations. A secondary depression and its "dying" parent would perhaps bear a similar relation one to another. In all cases the principal supply of heat would be borne from equatorial regions by warm currents of high water vapor content. Given sufficiently complete synoptic weather charts of the Northern Hemisphere, to test the above theory by a combination of the facts of observation, hydrodynamics and thermodynamics would not, I think, be impossible, though the task would be one of some magnitude.